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DDI #3661-82
30 April 1982

30 Apr 82

MEMORANDUM FOR: Director of Central Intelligence
Deputy Director of Central Intelligence
Executive Director
Chairman, National Intelligence Council
Associate Deputy Director for Intelligence
National Intelligence Officer for Strategic Programs
Director, Office of Soviet Analysis
Director, Office of Scientific and Weapons Research

FROM : Deputy Director for Intelligence

SUBJECT : John Prados Book The Soviet Estimate

1. I have spoken to several of you already about Prados' book on the history of US intelligence estimates on Soviet strategic forces. I believed after reading it that it was a reasonably fair minded account -- and at times an insightful one -- of the history of the strategic forces estimate. I further recommended it to several of you for whatever lessons we might take from it in terms of improving our record with respect to predicting Soviet force capabilities, both quantitative and qualitative.

2. In order to double-check my own limited historical knowledge, I asked Howard Stoertz to review this book for all of us. As you may recall, Stoertz worked in ONE on the Soviet strategic forces estimate from the early 50's until well into the 60's and then was NIO for Strategic Programs for seven or eight years. There is probably no one still available with a better first-hand recollection of the events covered in Prados' book. I attach to this memo a letter to me from Howie giving his review of this book. I commend it strongly to your attention. I would note his comment at the outset that "it should be recommended reading for all analysts and estimators working in the field of Soviet military affairs; and it would be of interest to those involved with Soviet affairs and estimating in general."

3. I recommend: (1) as Stoertz suggests, that all analysts be encouraged to read this book; and (2) that [redacted] and [redacted] get together to see what might be done in the way of organizing a small (3-4 people) working group to see how we can profit from this book and another work that Stoertz commends highly

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on the same subject. I do not think that anything like this has been done in the past to try and figure out why we were right when we were right and why we were wrong when we were wrong. Paul Nitze tells me that perhaps the main message of these books is that this uneven quality is the best level of performance policymakers can expect from intelligence on such matters. He may be right but I do not believe we can accept that without making some effort to prove him wrong.

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Robert M. Gates

Attachment:
As Stated

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30 April 1982

SUBJECT: John Prados Book The Soviet Estimate

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26 April 1982

Dr. Robert M. Gates
Deputy Director for Intelligence
Central Intelligence Agency

Dear Bob:

At your invitation, I have reviewed the recently-published book on "The Soviet Estimate," by John Prados (The Dial Press, New York, 1982). Along with another, earlier book, it should be recommended reading for all analysts and estimators working in the field of Soviet military affairs; and it would be of interest to those involved with Soviet affairs and estimating in general. The other book is "US Intelligence and the Soviet Strategic Threat, by Lawrence Freedman (The MacMillan Press, London, 1977). Prados covers the major issues, estimates, and debates about Soviet strategic nuclear forces from the 1950s through the first two or three years of the Carter Administration. Freedman's cutoff date is the end of the Ford Administration.

Both books are based largely, if not entirely, on unclassified sources, supplemented by interviews with some participants, recipients, and critics of the estimates. Neither author appears to have had access to the actual texts of the estimates themselves, except insofar as some limited excerpts have been leaked, released, or declassified, and thus have entered the public domain. Since much of the material now in the public domain is from such sources as DoD posture statements, Administration policy papers, and the Congressional Record, a great deal of the material available to the authors is about the uses of the estimates and about reactions to them, rather than about the way they were produced and debated within the intelligence community itself. Nevertheless, it is amazing how much of this long estimative history can be reconstructed successfully by scholars who did not themselves participate and whose sources about the issues and documents were mostly indirect. One wonders whether the classified history of the US-Soviet arms competition, which has been in preparation in the Pentagon for these past five years or so, will be as informative.

The very title of Prados' book illustrates an interesting and in some respects disturbing phenomenon about the focus of both US intelligence and US policy over the past 25 years. "The Soviet Estimate" is a catchy title but a grossly inaccurate description of the content of the book. Its content is actually limited to the more carefully chosen wording used by Freedman --

The Soviet Strategic Threat. Largely ignored in both works are the many estimates issued over the years on Soviet general purpose forces, on Soviet foreign policy, and on various other aspects of the Soviet challenge. Yet it is in fact true that the NIEs on Soviet strategic nuclear forces came to be regarded as the Soviet estimates, as Prados' title implies.

There were a number of reasons for this phenomenon. Partly it was because these particular NIEs had great continuity and regularity -- they were issued every year without fail. Partly it was because they were quite comprehensive, laying out much evidence and argumentation so that all could join in the debate -- and join in they did, often in the public prints. But partly also it was because both estimators and recipients came to expect these estimates to set the tone of the US interpretation of Soviet strategy and policy more broadly, sometimes in the absence of other major NIEs on the Soviet Union in any given year. Finally, US defence policy focussed quite heavily -- and wrongly, many have argued -- on the strategic nuclear balance. It is certainly understandable that the US has relied on strategic nuclear forces to compensate for shortfalls in conventional capabilities, rather than compete at a disadvantage with, say, Soviet ground force superiority. But in light of present US and NATO problems of strategic force modernization, and of the growing public sense of the dangers inherent in nuclear war, a case can be made that both intelligence and policymaking officials allowed themselves to be mesmerized far too long by the neat numbers, dates, and terminology associated with strategic forces and the strategic balance.

It is a humbling experience to read at one sitting a history of 25 years of US intelligence attempts to forecast Soviet strategic programs and forces. The number of cases where we got it just right are few in comparison with the numerous instances of substantial overestimates and underestimates on critical issues. Indeed, it was not until the SALT agreements set limits on total numbers of delivery vehicles that even the problem of the future magnitude of Soviet strategic forces was solved -- not by intelligence but by negotiation. Prados and Freedman review the obvious instances of substantial forecasting error at considerable length -- the overestimates of the bomber and missile gap episodes in the late 1950s, underestimates of the growth of Soviet ICBM and SLBM forces in the 1960s, underestimates of the rapidity with which the Soviets would improve ICBM accuracy in the 1970s, and generalized charges of belated recognition of Soviet war-fighting strategy in both the 1960s and 1970s.

Both books have these stories about right, and after all, they are fairly well-known stories by now. Prados is the more lively reading. He names more names and he has unearthed more anecdotes, like the fascinating vignette about how in early 1960 Stuart Symington conned Allen Dulles into confirming the "missile gap" by acknowledging that 35 (the estimated number of

operational Soviet ICBMs for mid-1961) was three times 12 (the planned number of US Atlas ICBMs). But by the same token, Prados makes more mistakes. Also, it is a pity that most of the names named are those of intelligence consumers, commentators, and dissenters. The people who really did the hard professional analysis of complex sets of data, and whose analyses finally corrected estimative errors or solved estimative disputes, usually didn't get their names in print and hence go unrecognized. Thus to anyone who was in the trenches at the time, the record of the ABM debate is incomplete when it fails to mention the role of [redacted] who slogged through to a correct analysis of the capabilities and limitations of the Soviet ABM and SAM systems -- and equally important, was able to explain the highly technical analysis as well as the uncertainties in it to non-technical policymaking officials. Many other examples could be cited. STAT

Working as he did with partial and indirect information, Prados exhibits a troubling tendency to mix excellent insights with the dark suspicions about the motives and actions of intelligence officials involved in the estimative process. DCI Colby's acknowledged effort in the mid-1970s to help the US see that the Soviets had a real stake in detente ("I used intelligence to try to expand the subject under debate from narrow weapons counts to the politics of overall Soviet policy") is interpreted somehow as making him a prejudiced adjudicator in the dispute over the range of the Backfire bomber (p. 264). Indeed, on that same page, CIA is alleged to have given its technical contractor, McDonnell Douglas, "accurate but incomplete" information for its analysis of the Backfire, and thereby to have led the contractor to a relatively short range estimate. These false allegations could only have come from a dissenter to CIA's findings. Colby himself, and DCI Bush after him, in fact judged that the quantitative data could support either a shorter or a longer range estimate. For this reason, two range estimates were presented in the NIEs without the expression of a DCI preference. But both Colby and Bush came down firmly on the view that the mission of the aircraft was for peripheral (i.e., Eurasian) and anti-shipping strikes, rather than for strikes against the US. They based that judgment on other, non-quantitative information. Nevertheless, the numbers drove the policy position, and as Prados points out, the US became committed in SALT II to treating Backfire as a heavy bomber until the Soviets made a last-minute accommodation in the negotiations. And the Backfire range issue would certainly have remained a stumbling block to SALT II ratification in the Senate.

Prados' treatment of the issue of Soviet ICBM accuracy and its impact on Minuteman silo vulnerability is too spare to be informative. Quite possibly this is because relatively little information about it had reached the public when Prados was writing in 1980. In this instance, Prados' tendency to be suspicious about the influence of outside pressures on intelligence surfaces again. On p. 257, he suggests that a B-

Team "warning" in 1976 that Soviet accuracies could be much better than then estimated caused intelligence to make a subsequent change in its estimate. Actually, the B-Team had been quite unable to shake the CIA analysis of Soviet ICBM accuracy. That analysis, incidentally, had no dissent within the intelligence community, and neither did the subsequent change.

What really happened was that intelligence had long predicted that the Soviets would in due course achieve ICBM accuracies sufficient to threaten Minuteman silos, but much technical evidence and analysis made it quite confident that the SS-18 and SS-19 systems being deployed in 1976 did not then pose a severe threat. Based on past Soviet practice in converting to MIRVs, it was estimated that new and better guidance systems would be installed on new missile systems anticipated for testing in the early 1980s. Instead, the Soviets installed them on modified versions of the existing missile systems, and testing began in 1978. The analysis of hard data from these tests, with no outside pressure whatever, caused the change in the estimate. A more complete and accurate description of these developments appears in an article by R. Jeffrey Smith, entitled "An Upheaval in US Strategic Thought," in the 2 April 1982 issue of Science magazine.

The suspicious cast of mind affects Prados' evaluation of the NIO system, about which he evidently could learn relatively little from unclassifed sources. In a brief and obscure reference, on p.293, Prados claims that "certain manipulations of the NIO system" occurred in a number of instances -- the SALT "hold" items, the Backfire range dispute, the B-Team episode, and the case of intelligence differences regarding Soviet progress in directed energy weapons. TheBackfire and directed energy debates were actually instances where the NIO resisted pressures to compromsie CIA analysis and was backed up by the DCI. The SALT "hold" items were instances of White House restriction on the distribution of information which, after a time, was resisted at many levels within CIA and was finally resolved by DDCI Walters' intervention with Dr. Kissinger, who had initiated the "holds." The B-Team experiment was resisted by DCI Colby but subsequently approved by his successor, DCI Bush. Prados alleges, on p. 294, that corporate integrity (as in an ONE) and policy responsiveness (as with NIOs) are at odds, and implies that a corporate ONE might have been able to avert the alleged "manipulations." He misses the point that over the years, whether the responsibility for drafting estimates was under the aegis of ONE or the NIOs, DCIs have exercised their prerogative to adjust NIE drafts and to make decisions of intelligence policy as they see fit, because that is their job.

Prados correctly calls attention to the influence that estimative successes and failures have had on the intelligence community itself, including its very structure. Among the factors leading to the establishment of a centralized Defense Intelligence Agency in the early 1960s were the intra-military

intelligence disputes and the patentlly self-serving Air Force Intelligence exaggerations during the bomber and missile gap controversies. Among the factors leading to the abolition of ONE in the early 1970s was its record of underestimating Soviet missile growth and the perception that it had become so wedded to previously-held positions that it failed to modify these positions in the face of facts. Among the factors leading to the suspension of PFIAB's operations during the Carter Administration was the B-Team episode and the perception that PFIAB had been more interested in promoting its own views about Soviet policy than in constructively critiquing estimative methodology. Perhaps more important, the credibility of intelligence, and its ability to assist the policy process, is to some degree a function of its estimative record as perceived by incumbent Administrations.

The record of intelligence is partly in the eye of the beholder -- that is, it depends in part on his expectations. The mixed record of the past 25 years of Soviet strategic estimates raises the question of whether US policymakers should consciously lower their expectations of accuracy in intelligence forecasts. Granted that this particular field is one in which US lead-times are very long and costs are very high, and policymakers naturally reach for all the help they can get. Granted also that this tends to be a qualititative field -- no matter how cautious and contingent the language, or how many the caveats, it is the numbers and dates that get used in the planning, and that go into the record, where they haunt the estimators and delight the critics, especially those with a penchant for computing percentage of error.

On this score, however, Prados has come up with a couple of interesting quotes from policymakers. One is from Admiral Moorer, former Chairman, JCS, who said in 1972 that beyond two or at most five years, all that intelligence could be expected to do was to postulate the kinds of programs that might be technically and economically feasible and strategically desirable for an adversary to pursue (p. 199). The other is from former Defense Secretary McNamara, who according to Prados frequently cautioned that intelligence was being asked to anticipate decisions the Soviet leaders themselves might not yet have made (p. 198).

Policy officials with the relatively modest expectations implied by these remarks might well view as adequate the intelligence warnings since ths 1960s that the Soviets would develop hard-target MIRVs, together with the predictions since 1974-75 that Soviet ICBMs would pose a severe threat to US silos in the 1980-85 period. Officials with greater and perhaps excessive expectations, on the other hand, might regard intelligence as having failed when, in 1978, it had to advance its estimate of the threat from the latter part to the early part of the 1980-85 period. The point is, that policymakers need to be helped to understand the strengths and limitations of intelligence forecasts, and that this understanding is not likely

to come from a set of numbers or even from words in a document. It is most likely to come from personal interchange between a responsible intelligence officer and a policymaker who has enough confidence in the intelligence officer to be receptive.

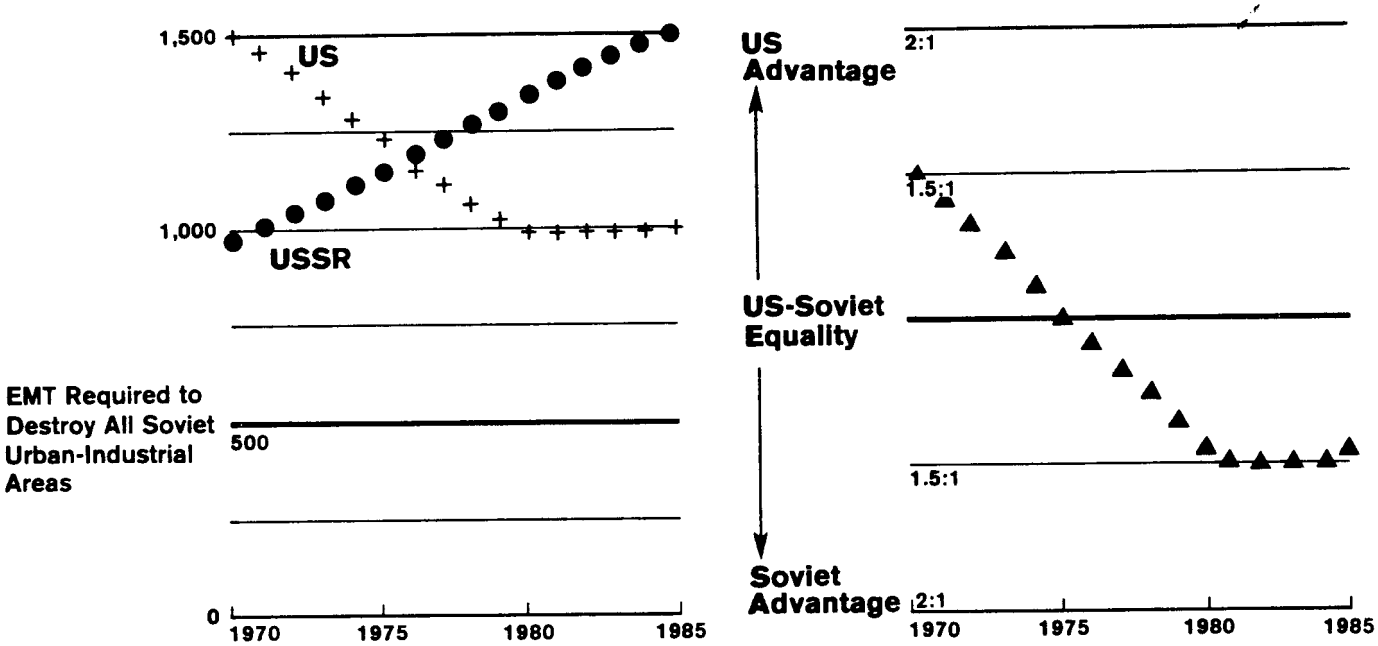
Curiously, one issue of intelligence policy which burned fiercely during the Carter years, and intermittently before that, has escaped Prados' attention. That is the issue of whether NIEs and other intelligence studies should contain comparisons of US and Soviet strategic forces and net assessments of the outcome of nuclear strikes or exchanges. This issue was fought partly on jurisdictional grounds. The military intelligence agencies, whose responsibility was limited to providing "Red" inputs to comparisons and net assessments made by their service chiefs and the JCS and OSD, held that the proper role of intelligence stopped short of assessing "Blue" forces. (This, by the way, was Allen Dulles' position when he tried to avoid saying who was ahead during the "missile gap" dispute.) NIOs and DCIs, especially DCI Turner, held that comparing Soviet strategic forces with those of their principal adversary, which happened to be the US, was necessary in order both to forecast Soviet programs and policies and to make the forecasts meaningful to their US recipients. They could cite both Administration and Congressional desires to get such comparisons from the DCI.

A second aspect of this issue, and perhaps the more important aspect, was the question of the content of the comparisons and assessments. Crudely put, it was whether the message of the assessment with respect to US policy would be mainly reassuring or mainly alarming. This depended to some extent on what was selected to be measured and how the measurement was displayed. By way of illustration, on the next page are two charts, much oversimplified, each containing identical notional data on the equivalent megatons which might remain available in US and Soviet strategic forces after a preemptive Soviet strike against the US Triad. The left-hand chart is typical of that which DCI Turner used in NIEs and Congressional briefings. The right-hand chart is modelled on the type of presentation which appeared in JCS posture statements and some other Pentagon issuances. The contrasting implications about what was important are clear. The NIE presentations, while revealing Soviet gains, tended to stress that both sides had enormous power and that even the residual US power would be sufficient to do a certain job and then some. The contemporary military presentations stressed that the Soviets were forging ahead. The contrasting policy implications are also clear. The NIE presentations lent support to those who believed that US programs could be geared to sufficiency in the conviction that strategic deterrence was relatively stable, and who urged greater emphasis on conventional capabilities and other priorities. The military presentations lent support to those who believed that Soviet strategic superiority was an imminent danger which called for urgent measures of rectification.

Contrasting Presentations of the Same Data

Equivalent Megatons Remaining to US and USSR After Soviet Preemptive Strike

(Data and Scales on Charts are Notional)



The foregoing dispute has evidently been resolved, at least jurisdictionally and for the moment by a decision that NIEs will not include comparative data and that CIA will participate, with others, in a more comprehensive net assessment managed largely by the Pentagon. If it had been included in either Prados' or Freedman's books, this dispute would have provided excellent additional support for their conclusions. In both books, the conclusions are analytical rather than prescriptive. They are fairly modest and fairly similar, though Freedman's concluding commentary is the more scholarly.

Prados concludes that his study reveals "The odd tangle between intelligence and policy" (p. 291). He compliments the analysts and estimators in a way not usual these days, by noting that in most instances the main lines of argument about a particular aspect of the Soviet strategic threat were laid out and the main elements of evidence were successfully collected, often years before the threat actually materialized as an operational capability. He notes as well, however, that it was also often years before the resulting intelligence disputes were resolved. He recognizes that there is a degree of what he calls "residual uncertainty" in any intelligence estimate, warns of the danger that estimative judgments and the estimative process can be manipulated by bureaucratic and political pressures, and winds up with the observation (hardly a revelation) that wise policies must be based on more ingredients than just intelligence (pp. 295-299).

Freedman, too, stresses the interrelationship between intelligence and policy, but he does not find it odd. He concludes that "intelligence estimates derive their salience from the broad frame of reference adopted by policymakers to deal with strategic arms issues" (p. 190). He notes that the "adversary image" held by the policymaker is a product of his total experience, and is often his basis for judging. The perceptions of the authors of intelligence estimates rather than being subject to modification by those perceptions. In Freedman's opinion, "the process by which threats come to be perceived is a political process, in which the nature of the prevailing strategic doctrine in the US, and attitudes toward the defense budget and arms control, are as crucial as estimates on the evolving Soviet force structure. When there is a deep split within the Administration on the fundamental direction of US strategic arms policy, there will inevitably be a deep split in the perceptions of the threat (p. 198). No doubt Freedman would agree that this goes for an alliance or a society as well.

The common message, of course, is that intelligence estimates are neither produced nor received in a vacuum, that they are relevant to policy only when they address issues perceived to be of major concern by policymakers, and therefore that they both affect and are affected by the policy debates animating the Administration, the Congress, and the informed public. Is this a situation to be deplored and avoided,

warranting strenuous corrective attempts to isolate intelligence analysts and estimators so as to enhance their objectivity? Certainly not, even if it were theoretically possible. The reason is in considerable part because the Soveit policies and programs about which we are estimating are not conducted in a vacuum either. They spring from both internal imperatives and compromises - often involving very complex interworkings -- and as well from external stimuli. Among these, the stimulus of the US-- its programs, its policies, and its stance toward the Soviet Union -- is clearly one of the most important. To improve our estimates and analysis of Soviet strategic programs and policies, therefore, each of our analysts and estimators should be encouraged to learn more --not less -- about how his particular piece of the puzzle fits into the larger picture of the hopes,, fears and strategies of both the USSR and the US, and indeed of other key actors such as the NATO allies and China.

From this perspective, integrating the force analysts with the poltical, economic and foreign policy analysts in an Office of Soviet Analysis is a good move, whereas backing away from US-Soviet comparisons and net assessments in NIEs and other intelligence studies is not.

Recognizing that each NIO is heavily engaged, both intellectually and in terms of time available, in keeping up with his own special account, it would nevertheless be highly desirable to insist that there be a close working relationship among them. This would be especially useful for the NIOs responsible for various aspects of Soviet affairs, including strategic forces, general purpose forces, and national political and economic policy. Among this group, collegial review of work programs and draft estimates should be mandatory.

Finally, it seems inexcusable that the position of NIO for the USSR should remain unfilled. To whom can CIA's many analysts and estimators turn for the "adversary image" against which to test their piecemeal findings and judgments and to integrate them into a broader frame of reference? If that broader interpretation does not come from within the CIA, it will surely be insinuated from somewhere, and its sources are very likely to be less well qualified, if not more heavily influenced by policy preferences.

Thank you for the invitation to review these interesting materials and to comment on their implications.

Sincerely,

Howie Stoertz